543

INTERVIEW WITH JIM DOWNEY
INTERVIEWED BY STEPHEN P. WARING
14 DECEMBER 1990
MADISON, AL

1. DOWNEY: ...I am not a person that keeps diaries or personal logs or anything like that, but you will find little in the early days of the work that we did on the HEAO program. It was inspired by local initiative. We bootlegged the work. I hope that you can find a more diplomatic way of expressing that perhaps. But, it was not like we had a big authorized project that had come down from NASA headquarters. A group, myself, Gilruth and a couple of other guys, got the work initiated over in Space Sciences Lab. First, the project was known locally as the EMR Project, standing for Electromagnetic Radiation. It in a way...well let me backup. I will tell you, if this appropriate at this point, I will tell you how the project work was started. I, indeed, was involved in that. Ernest Stuhlinger had a meeting with Dr. von Braun and they were talking about...von Braun was of course very interested in astronomy and science. You know the Center has always been involved in doing scientific projects. But of course our reputation is a Center of launch vehicles and propulsion. But throughout the whole history of the Center there has been an active set of science projects under way. At any rate, von Braun had talked to Dr. Stuhlinger about maybe doing some conceptual work on an optical astronomy mission.

2. WARING: When was this, 1968 or 1969?

3. DOWNEY: I believe it was earlier than that. I think that it would be more like 1965. Yes, I am sure that it was earlier than that. That is one of the things that I will ask you to talk to Dale Ruth about, because he may be able to point some of the chronology of his career better than I can. But I would say it must have been around 1965. But I was concerned at that point when Ernest Stuhlinger asked me to look into the...at that time, by-the-way, I was heading a group, I believe they were calling the Special Projects Division

down in Space Sciences Lab. We were an outgrowth of a little bit of the old Electrical Propulsion Program that had been transferred to Lewis Research Center. To digress a minute. Marshall had a very, very active program in electrical propulsion and Ernest Stuhlinger was the focal point of that program.

4. WARING: What do you mean by electrical propulsion. Is that the nuclear?

5. DOWNEY: No, no, I am talking about ion propulsion. We also looked at the same time, they were looking at some plasma propulsion techniques. The Center was involved in the nuclear rocket program, the NERVA Program. The work in SSL under Ernest Stuhlinger was focused on nuclear electric propulsion and ion propulsion, but as I mentioned we did do some work over there in plasma propulsion also, or jet propulsion. But there was a very active program in the sixties on nuclear electric propulsion in the SSL and I am sure that you will want to cover that in your history. Ernest Stuhlinger would be the man. He is a very viable and active fellow still. The electrical propulsion program was near and dear to Ernest's heart. But a decision was made to transfer the work form Marshall in electrical propulsion to the Lewis Research Center. That was accomplished...by the way, I hate to offer to do this, but I will, I may have some old documents in my basement that summarizes the Marshall Electrical Propulsion Program.

6. WARING: That would be great!

7. DOWNEY: The reason I say that I hate to volunteer to look for it that it is such a mess down there.

9. DOWNEY: That is an important part of the Space Sciences Lab background was its work in electric propulsion. I was involved there, but then I headed up a branch that later became a division under Stuhlinger. We did a lot of work associated with the post-planning for the lunar surface program, the Apollo Lunar Surface Program. We got into a lot of that. But as part of that same work, we became involved in that suggestion that resulted, ultimately in the HEAO Program.

But to get back to the origin of that, von Braun had asked Ernest to get some work done looking at an optical astronomy payload that might Marshall be able to recommend. At that time Goddard had a very active program going, not optical astronomy, the OAO, Orbiting Astronomical Observatory. It ultimately flew just four missions. That was a very, very major program in its day and a lot of technology and very challenging project that Goddard was involved in. So after some discussion with Ernest, we decided to go back to Dr. von Braun with a recommendation to look into a system involving, not optical astronomy, but X-ray and gamma-ray astronomy. I remember the presentation that I gave to Dr. von Braun. It was no big affair, it was just two or three of us in his office, Ernest, myself and Dr. von Braun. There may have been one or two other people in there, but I really don't recall. We suggested to von Braun and he pretty much jumped on that suggestion, that the reason being, some of those payloads that we envisioned in x-ray and gamma-ray astronomy, to accomplish those experiments would require a rather massive payload. In those days, most of the scientific satellites that people though about were rather small. The Saturn system was being built. But at any rate, we at Marshall said, "Hey, we know there can be big rockets to launch big scientific payloads and why do we go ahead and boldly suggest a rather major scientific payload that has considerable mass."

10. WARING: Can I stop you right here? Was part of the decision to move away from optical into x-ray and gamma ray astronomy so that you wouldn't step into the turf of

Goddard?

11. DOWNEY: Yes, at that time. Also I think that not so much the turf, but the fact that the agency already had a major payload program going in optical astronomy.

12. WARING: And they wouldn't fund two?

13. DOWNEY: And I wouldn't think that they would fund two. I don't really know what Dr. von Braun's thoughts were. Maybe he was envisioning a large space telescope at the moment. But at any rate, we came back with this suggestion saying...I will have to admit that my thought process at that point, Goddard wasn't doing anything in conceiving a major observatory to work in the x-ray and gamma ray regime. So I think that the combination...the main thing was that the agency already had a big program, to me, a big program going in astronomy with the OAO program underway. So we said if we are going to do something that somebody is going to be interested in, let's conceive something different from that.

14. WARING: Okay. Was it also part of your consideration in going into a large payload that it would provide more work for the Center in a time in which the Center was finishing up work on the Saturn project?

15. DOWNEY: No, I don't think that was the case, really. It certainly wasn't in my mind when I came up with that idea. The very earliest idea of that proposal. It was more that there was some people out there in the x-ray and gamma ray community that want to do big things with flying massive payloads and we at the Marshall Center have a vision that we can put massive payloads up on scientific satellites. I guess it was that we had the big rockets and big boosters. We thought big, perhaps. I had heard a lecture and I even forget when I

heard the lecture and a person was talking, it was a scientific discussion and they said such an experiment would weigh five thousand pounds and that would be inconceivable to do an experiment of that mass in space. I thought, yes you can't put it on today's sounding rockets, but maybe that isn't inconceivable. The rocket people were thinking big, the scientists were thinking little rockets at the time. So one of the things were that we brought the scientists to thinking and the people that knew something about big rockets, bring them together and see if there isn't some common ground.

16. WARING: When you attended this paper, was it a paper delivered here in Huntsville?

17. DOWNEY: Yes, it was here in Huntsville. I really don't remember who...it is all very hazy now. Even when I suggested the earliest version of the HEAO, it was still part of the thought process. Again, and I am sure that there other groups around the country that had similar thoughts at the time we did in the Space Sciences Lab. Dr. Riccard Giacconi a person very well know in x-ray astronomy was obviously thinking about things of the same kind of nature that we were. Riccardo wasn't where the big rockets were though at that point.

From that, when we met with von Braun, he was...again, please be discreet with history, because as I recall his comment was, "well, we don't have any market work on this stuff and I don't want you to come running around with some half-baked thing that doesn't make any sense. Can't you just quietly go back and study it for a while. Don't come up with some premature proposal. Let's put some meat on the bones. Let's go out and work it a bit. It sounded like a good idea. Don't embarrass us, don't get us in trouble. Just quietly go to work." That is why you won't find too many things in the early records, we just quietly went to work. Of course we quickly ran to the end of our resources as far as the conceptualizing of instruments was concerned, so we started talking to some scientists on the outside. We talked to a group at Oak Ridge National Labs who had interest in a

gamma-ray experiment. Obviously they had the instrumentation technologies and techniques coming out of their work in the Atomic Energy Commission work. Of course now, it is the Department of Energy. In the early days that group was headed by Dr. Jack Gibbons.

18. WARING: This was 1967, '68, '69 period?

19. DOWNEY: I think that it was more like '66 or '67. That is why I hope you will be able to get in touch with Dana. Maybe he can get these dates a little bit better. You know we worked unofficially and "catch-as-catch-can" for quite a long time. A year or so. No money, just bootlegging the output, catching it when we could. There was a great upwelling of enthusiasm. Whenever we talked to anybody in the science community about the possibility, they got very excited.

Jack Gibbons, as I said, headed that group at Oak Ridge at that time. You probably aren't acquainted with that name, but years later he handled a national post and head of the Office of Science and Technology Policy, if I got the name right. He went up away from his hands-on science career and became a policy-maker, a scientist working at the highest policy levels in government.

20. WARING: Did the HEAO Project get a new initiative when Program Development started? Was that what converted it from this informal project into something more formal?

21. DOWNEY: Yes, it got a little more formal as...I will try to give you one or two other names as the time conceptual phase was being completed, I faded away from it. Dana Ruth faded away from it and other people began to take over that responsibility. I mentioned the first name that we had was called it EMR (Electromagnet Radiation). I

forget who came up with that, I certainly didn't, it just started being called that. Of course, optical light is electromagnetic radiation as well as x-rays and gamma-rays. Any rate that was what it was called for a while EMR. Then, when we did expose our plans to NASA Headquarters, Office of Space Science and Applications, Dick Halpern became involved. I can give you his current telephone number if you wish.

22. WARING: Yes, he is somebody that we need to talk to anyway.

23. DOWNEY: I still work with him in totally different arena now, of course. Area Code 202 554-0269, that is his number at work.

24. WARING: Is he still at NASA?

25. DOWNEY: No, he is now retired from NASA and is with a private firm in Washington. Dick was the manager at headquarters and one of his first actions was to change the name from EMR to something else. He changed it to Super Explorer, with the acronym SEX. That rapidly came into disfavor! Out of all that came the ultimate name, High Energy Astronomy Observatory or HEAO.

26. WARING: What was the reception of Headquarters to the project, this initiative that came from a center that didn't really have a big a reputation in Space Science as Goddard?

27. DOWNEY: It was interesting. Frankly, they were very upset initially.

28. WARING: "They" meaning who?

29. DOWNEY: The guys in the Office of Space Science and Applications. I say they were

upset, we were out of channel. Dr. Nancy Roman, another famous person now, retired, was head of the astronomy program in NASA at the time. Let me say though, I say they were upset, but I don't mean they persecuted us or anything like that, they knew that we had been out of channel a little bit. But the attitude was quickly turned to, well I don't know why somebody hadn't been working this sooner. Their originally reaction was what in the world has been going on?" to they got in there and really provided the support that we needed and became advocates and helped us with the conceptual definition of the program.

30. WARING: How did Goddard react?

31. DOWNEY: Their initial reaction was also one, a little bit of why didn't we do it. But Dr. Frank McDonald at the Goddard Center became a very important person in HEAO. He supported us. Others did too. Dr. Frank McDonald became the mission scientist on Mission One. So we had a collaborative effort with Goddard. I think Frank, I don't want to put words in his mouth, but I think Frank attitude was...by the way, he was interested in, Frank is, I believe primarily a gamma-ray astronomer, although he had as his career evolved, of course he had much broader responsibilities and had high-level scientific post in NASA. I believe that Frank was originally and primarily a gamma-ray, interested in gamma-rays. So his attitude to me was, "well, you guys, we will commend you for thinking of this and pushing it and I want to work with you, I don't see why Goddard didn't think about it." So he was kind of maybe miffed that Goddard hadn't thought about this or taken the initiative to move out. But then Goddard had their plate full. They had a very active set of things going on.

I may have to back up a minute though to say that we at Marshall rapidly ran out of abilities to proceed without talking to people and working with people in the science community. I mentioned we went and got with the Oakridge people, with Dr. Jack

Gibbons. Dr. Herb Freeman, we began to work with him at the Naval Research Laboratory, very renowned scientist as well. Then we also...at that point I got the idea...well, I said, "There maybe some strong coupling between things could be observed in ultra-violet and in the x-ray." We began to work with the University of Arizona a little bit to get some ideas from them in the ultra-violet area. The person out there that we mainly dealt with was Dr. William Tiff.

32. WARING: So while you were conceiving the idea about a new sort of scientific satellite, you seeking advice outside of NASA and in effect building a coalition to get the Agency to support the project?

33. DOWNEY: Well, we really weren't that sophisticated! We didn't think we were building a coalition. We weren't trying to build advocacy. We were just trying to get some ideas so that we would have a respectable proposition. In order to define the payload, we needed to have better insight into what experiments might fly on it. What would be their characteristics, their masses and so forth. By the way, the ultraviolet was dropped from the consideration as the project matured. The payload was downsized at some point downstream. But I am getting ahead of myself. It ended up flying on three separate missions. So, the payload went through a considerable evolution from its earliest plans. Now what has bothered me at this point, I mentioned the people that we talked to early on. The earliest people that we contacted, William Tiff and Gibbons and Freeman and there was at least one other scientific group that we were dealing with and then it began to broaden. For example, shortly thereafter we began to work with Dr. DeConey became active with interfacing with us. Of course his organization has formable credentials and capabilities. He was at that time with the American Science and Engineering Company. They ended up, of course, flying the principle payload on HEAO-2, which was a focusing x-ray telescope.

Our earlier days when I was working on the proposition with Dale Ruth and we could just...we were just thinking of a satellite that would have these massive, heavy payloads requiring various degrees of shielding and so.

34. WARING: And launching it with a Saturn I booster, perhaps?

35. DOWNEY: Perhaps. We were thinking in that kind of size. Not requiring the full capability of...we were thinking of a payload in the 40,000 pound class originally. But we weren't thinking of a very sophisticated payload that would have to be accurately pointed. For example, we could more or less aim it at a certain region of the sky and start counting photons.

36. WARING: So, as I hear you talking about the ways EMR or HEAO was designed, essentially Marshall's role was that of an integrator. You had sort of taken ideas that were already out there and were helping to bring them together into fruition as a real on-going science experiment.

37. DOWNEY: Right. We were taking some ideas that the science community had and trying to maybe help shape those ideas in the sense that Dr. Herb Freeman had been flying NRL, Naval Research Lab, had been building these smaller x-ray proportional counters. Basically, what we can accumulate is a function of the area of his counter, his proportional counter. The time that is exposed in space. So we were suggesting that you could put a massive array out there, a big area. So I think that we simulated some thinking that yes, maybe you could do that. With a massive array you could count more photons and have a better chance of discriminating against background radiation. The bigger area you got, the longer you can count. So I think we did shape some of the thinking of the science community by maybe leading people to thinking bigger than they had been thinking.

38. WARING: Besides that sort of encouraging the scientists to think bigger, were there areas of expertise at Marshall that particularly contributed to the development of HEAO? Were there skills that Marshall had that did not exist elsewhere?

39. DOWNEY: No. At that time, from the standpoint of experiment design and development, experiments, I really can't think that Marshall contributed. Although we...I want to correct that. We had some considerable capability in the Space Sciences Lab, with Dr. Russell Shelton's group, to do radiation transport calculations, shielding and radiation transport calculations. They really had a strong group to do that and they certainly contributed in that area.

40. WARING: Why did Marshall have that kind of capability?

41. DOWNEY: Well, they just build up around the nuclear rocket program and concerns about the shielding requirements with nuclear rockets and several people working with Russ Shelton were physicists and mathematicians who had originally even worked with the Atomic Energy Commission. That is a word associated with the earlier aspects of the AEC endeavor. So, that was a rather unique area that we had capability in at Marshall. But as far, we did not have anybody at that time that was a practicing x-ray astronomer, for example. Or gamma-ray person at Marshall. Now, of course, that situation has changed. We now have rather strong science, specific, but focused strong people at Marshall in x-ray astronomy and gamma-ray astronomy. But at the time that I am talking about in the mid-sixties or so, we did not.

After this proposition...see when headquarters got involved the old EMR project really became the legitimate and recognized. Then they began to work like a tradition science program. I emphasis again, our earlier concepts with the science community were

made so that we could conceptualize a payload that kind size it in a gross sense of what...then when headquarters got involved, it became a legitimate NASA project and they began to go through a formal selection process of identifying principal investigators to fly on the missions. Through that process the ultra-violet was dropped from the payload. I think that was a good decision. Work associated with...ended with a payload involving cosmic ray physics and x and gamma ray. We ended up with three missions involving cosmic ray physics, x and gamma ray astronomy.

The first and third HEAO missions were missions that didn't require a high degree of pointing accuracy.

42. WARING: They were just mapping general areas?

43. DOWNEY: Yes, broader-field-of-view instruments. Instruments that didn't have to be so precisely pointed. But you needed to know grossly in what direction they were pointed. You wanted to avoid pointing toward the earth obviously.

The [238] that the program went through is a considerable evolution from its earliest conceptualizations down in Space Sciences Lab. As I may have stated, Frank McDonald became the first project scientist on the HEAO mission. He worked with us from Goddard and supported us at the center. His cooperation was greatly appreciated. Of course, it would have been more convenient if he had been physically located at Marshall, because there is a lot of project interaction between the project scientist and the project managers. But we did not have the science capability in those early days in these particular areas of science. That began to change in the mid to later sixties and the scientific competence at Marshall was increased significantly.

By the way, Dr. Tom Parnell...

44. WARING: Yes, I have already talked to him.

45. DOWNEY: Did he tell you when he joined us, because he was not working with me on the earliest conceptualization of the EMR.

46. WARING: No, I can't recall because it was last summer when I talked with him. But, I believe he came in the late sixties.

47. DOWNEY: Of course, we had a lot of stuff going and he was a godsend to us. That was our first project scientist that we had at Marshall. He was HEAO-3 as the project scientist.

Went to work with SSL as the conceptualization of the system was completed and things were turned over to headquarters. The program became a legitimate NASA program, other things occurred. Lee James at that time, was involved. The program came under him. I am not very good at calling these formal organizational titles, but Lee was responsible, under him, the HEAO came under Lee. Rodney Stewart, who is still in Huntsville, retired from NASA a number of years ago. Rodney Stewart was first manger of the HEAO program under Lee James.

48. WARING: At one point you said that faded out of the project. At what point did you fade out? About the time that it was beginning to be formalized?

49. DOWNEY: Yes just about the time that it was evolving from Space Sciences Lab, I believe in 1968 or so is when I was pretty much beginning to fade out of it. I still tagged along. But I was not longer responsible, or the main guy. Rod Stewart was leading it and Hans Stigma was his Deputy.

50. WARING: By that time there was an office Project Office?

51. DOWNEY: Yes, under Lee James, that is when the project office became official. There ought to be some official records on that to nail that time-wise.

52. WARING: Is Hans Figner still alive?

53. DOWNEY: Still very viable. I saw him on a travel log program two or three weeks ago, Kwainis Travel Log. He would be a could guy to talk to. He came out of Dr. Hauesserman's lab originally. He was just a good, good, solid engineer and manager in my view. So he was a stalwart behind this HEAO program.

54. WARING: As you have been describing the origins of HEAO, you are describing a very freewheeling process. Do you think that was typical of the period or is that typical of the origins of any science project at NASA? Do you think there was something unusual about the way that people could initiate projects back in those days?

55. DOWNEY: Yes, I think it was different then. I don't know if it was different because of the system or maybe we just didn't know what we weren't supposed to be able to do. Maybe we were just too young to be as easily constrained to a system. I don't know, but I don't think we could do it today.

56. WARING: There would be more red tape today? More channels?

57. DOWNEY: Today there would be much more tighter controls over the main power sources of the center. The project has to become kind of official before you can start working on it now. To me in those earlier days, we would create the project. The system was much less sophisticated, the institutional system, was much less sophisticated. To me it

contributed more to an environment of innovation and creativity. I think it tends to be stifled if you control people to too great an extent. I know it is a difficult balance to achieve. It would be hard to work HEAO today at Marshall in the Space Sciences Lab like we did in the mid-sixties.

58. WARING: It is more difficult for a NASA center to break into a new area now.

59. DOWNEY: I would definitely think so. The whole system up and down the line is controlled much tighter in my opinion.

60. WARING: Do you think that is because money is in shorter supply so they have to make sure that every engineer and every scientist is working every minute on the project that they are formally assigned to?

61. DOWNEY: I think that is a big part about it. I think also, NASA has matured as a bureaucracy over the decades of its existences.

62. WARING: Just more rules and more controls.

63. DOWNEY: More rules and more controls. I realize there has to be a balance between just completely free wheeling laissez-faire organization of people going out and working on anything they want to, versus a very rigidly controlled, every minute of every hour you work this project, you do that thing. Somewhere between those two extremes there should be a balance.

64. WARING: Do you think Program Development was way of striking a balance between that integration which is necessary to develop new ideas and more control which, maybe, is

necessary to keep them going in an era of tight funding?

65. DOWNEY: I think Program Development where all of that became focused and when Program Development was established under Bill Lucas in 1969. Of course that was Dr. von Braun's decision to establish a program development directorate and I think Dr. Lucas put in the architecture, the plans for program Development. I believe that was a brilliantly conceived organization by Dr. Bill Lucas. It did just what you said. It provided a focus and a little more legitimacy and control, but still didn't stifle the creative environment of being able to conceive new things and think new things. Moving to Program Development I felt that now you had a real home to do this work. You didn't have to bootleg it. I think it did achieve. I can't quite express or articulate exactly my thoughts on that basis. Von Braun took some of the people that had been working on the Banks Systems stuff and guys that had been off in places like Space Sciences Lab and the fellows that had been working with me and put all these fellows together...put these groups of people together in Program Development. I think that part of it was that he thought some people who had maybe demonstrated some... well, maybe it was two things. Maybe he was trying to control some of these mavericks and rope us together! But I think it was really some of the people that had demonstrated some enthusiasm for new things and had created some things, innovation and that got their juices flowing. He thought it would be good to put those kind of people together.

66. WARING: It seemed to me in talking with people about Program Development that people in Program Development thought of themselves as the entrepreneurs for the Center.

67. DOWNEY: That is right. I certainly felt that way when I moved form Space Sciences Lab to Program Development in 1969, I headed up a Mission and Payload Planning Office

in Program Development. We had a group of people...within PD we were more the science payload, scientific looking group. Then they had advanced systems people, preliminary design people and so forth to support. But my group was more concerned with innovation in the science arena, scientific payload, scientific projects. I can't quite really correlate the transition between HEAO because I think it was mainly worked, not in Program Development. I think that before Program Development was formed, Lee James took over responsibility. So the conceptualization, the PD job on HEAO was mainly or maybe totally done down at Space Sciences Lab. PD came along after the early work on the HEAO, during the period when it was called EMR and then SuperExplorer.

68. WARING: I will check the chronology on that. That would be a good thing to know.

69. DOWNEY: I don't know exactly how that happened. I know that when I went to Program Development I only was peripherally involved in HEAO and that was just kind of part of the transition thing to continue to keep touch with it. So I worked with Rod Steeler and Hans Spickner, but it was more of a supportive kind of thing as one of the old guys on the program not being any longer responsible specifically for anything.

70. WARING: Can we change from HEAO for a bit? When you got involved in the Payloads Office, under Program Development, did you work closely with the development of experiments for Skylab? What was the relationship with Program Development with the Skylab Project Office?

71. DOWNEY: Again, a lot of the early work on Skylab was done before Program Development came into existence in 1969. I remember when I was down in Space Sciences Lab I was involved in putting together a center proposal for the ATM (Apollo Telescope Mount). Others in the Center were also involved. Greg DeJaycee...people over in Dr.

Hauerssman's lab were involved in preparing that ATM proposal, as was I. Those people are still around and around town. In fact, one of the principals is still working at the Center. The original proposal was...the early work, the early principals that were pushing that were: Ray Easely, he was right in the midst of it, and Bill Horning. Bill was on the lab side and Ray was on the project side. I never was a central figure in the ATM proposal, like I was...we were supporting from SSL, not...we weren't the main energy behind the ATM activity. But Ernest Stuhlinger was very interested in ATM of course and he had a number of experiment scientists assigned to work on the ATM.

I remember my own role was I helped to put the original center proposal together for ATM. I supported Bill Horton and Fred Dejacy.

72. WARING: Can you tell me about that original proposal?

73. DOWNEY: It is hazy with the passage of time. All I can remember is staying late up two or three nights, working all night on it. Just putting words together and trying to emphasize the Center's abilities to...yes, we put together a pretty good proposal.

[End of first side of tape.]

74. WARING: ...this module? Or were by that time were already thinking about it being attached to a spent-stage workshop?

75. DOWNEY: You know I really cannot recall. I think that we were already thinking of the work being on a spent-stage though. After Skylab...are you familiar with the Apollo Applications Program?

76. WARING: Right.

77. DOWNEY: We were looking as a part of the Apollo Applications Program at the possibility of four major science payloads using old Apollo and a lot of the old technology Apollo derived hardware. One of these was the...well, we had the same old areas of astronomy, optical, invisible. We brought infra-red in also and of course the equivalent of ACTEF today, the big x-ray telescope. So we looked at those kinds of things...and solar. I was about to leave that out. That was a key when related to ATM. So we looked at all of those things as the Apollo Applications Program. The person that you ought to talk to about that and will remember it real well is Max Nein. Is that name familiar to you? He works in Program Development still in the office that I headed many years ago. Now they call it, I think, Advanced Orbital Systems Office. But he is in the Marshall Directory. He will remember what we did in the Marshall Apollo Applications Program. With that work we were definitely looking at something not like something on a manned service module. We were looking at a independent, major large observatory class payloads. Observatory class payloads are on the scale of a LST, Large Space Telescope for Hubble, or AXAF, or the SIRTF, that is Shuttle Infra-red Telescope Facility. It has not been approved yet for development. It is a post-payload. Same with the major solar observatory. It was not approved yet for development by NASA, but it has also been conceptualized and worked and even put under contract. Max would be a good to help with an understanding of the Apollo Applications Program and what concepts were involved in the astronomy areas that I mentioned.

I was not right in the thick of the ATM thing as I was on HEAO. I am trying to remember whether Rein Ise was involved in preparing that ATM proposal. I believe it was mainly...he might have come on after that. After we did the Center proposal. There we were all playing it very legitimate there. We were putting the Center proposition together, sent it to headquarters for consideration. I think Fred DeJaysu and Bill Horton are the guys who managed that managed that proposal preparation activity on the ATM.

78. WARING: With the work on ATM and HEAO, Marshall was diversifying its role in NASA. It was moving from being primarily an engineering center involved in propulsion to also being involved in the science side.

79. DOWNEY: Right and you know when you asked me a few minutes ago what did Marshall really contribute to the old EMR/HEAO science, although we didn't have the scientist...I mean, it is all engineering work. It is all challenging engineering work; structures, thermal, data systems, software. It is just a total engineering job. Of course the scientific groups around the country that do the experiment work still have the engineering staffs that put those payloads together to their requirements. These experimental physicists are basically tinkers and engineers too. I guess about have the radio astronomers in this country, don't have degrees in astronomy, they have it in electrical engineering. So I am just making the point that Marshall had the requisite technical skills, engineering skills to develop and manage a program like HEAO.

80. WARING: Would you say you felt like pioneers in the Center moving into something new or did you just feel that you were building on strengths?

81. DOWNEY: No, we didn't feel like pioneers at all. We were just building on strengths that were already there. We didn't think we were venturing into some unknown sea or something. If you go back to Explorer I, and of course, PEGASUS.

82. WARING: Were you involved in PEGASUS?

83. DOWNEY: Only peripherally. No really I was not. It was done mainly down in Space Sciences Lab in the early era. Dr. Bill Johnson was the manager of the PEGASUS project.

He is now deceased. Ernst Stuhlinger was in his lab. A person that you may have heard of, Jack Lee, worked PEGASUS. As I recall, Jack was the...PEGASUS was done under contract with Fairchild, and as I recall Jack spent some time at Fairchild early in his career as a young engineer as a resident manager or NASA representative at Fairchild. I maybe wrong on that, but I certainly have that recollection. [That] Maybe the first time I ever heard about Jack Lee.

Then there was a group of people down in the Space Sciences Lab that were deeply involved in PEGASUS. Bob Naumann, recently retired from NASA and is now with the University of Alabama in Huntsville, knew a good bit about that project. Marshall, that group, that Naumann was in, was very interested in meteoric impacts for two reasons I think: trying to understand what the ambivalent background, origin, composition, spectra sources, what the situation was with a meteor environment in space and lower earth orbit; and of course, with equally or greater importance, was the meteoroid environment...what is the effects of meteoroids on spacecraft structures and how do you design meteoroid bumper structures, structures that dissipate the energy in an impact, a bumper type device. That work was of great interest in the early period of 1960 or so down in Space Sciences Lab, then called Research Projects Lab. As the early work evolved they formed a project office under Dr. William Johnson. They went on to fly, I believe, three separate PEGASUS payloads. They gathered reams and reams of data. We didn't nearly have the technology available then as we do today to analysis data and store it. So there are just reams and reams and rooms and rooms full of the PEGASUS tapes. It was very time consuming to analyze the data.

Just to digress a moment I have been working on a study contract for Marshall and found it necessary to interview a number of people, so I know how time-consuming it is. I keep saying, why don't you talk to these people, but I am trying to point you to a few that will help.

84. WARING: This is very helpful because some of these people I will be interviewing anyway and if I see the context in which they work, I know what sort of questions to ask them to make the best use of time.

Could we move on and talk about the Large Space Telescope a bit. I want to come back and ask you questions about that when I have done more reading about it. But if we can see some connections between the Large Space Telescope and HEAO and that sort of thing. Provide a general background, right now. How did you get involved in the Large Space Telescope?

85. DOWNEY: Well, the work on the Large Space Telescope really started in Program Development. Anybody that has ever looked through an earth-based telescope and thought about it very much is obviously in an atmosphere of frustration and causes you all sorts of problems even on the clearest of nights. Anyone that has ever thought about astronomy very long has thought, "Goodness it would be nice to have such a system in earth orbit above the disturbing effects of the earth's atmosphere." I don't really know where the idea of the Large Space Telescope came from, there was an important summer study activity in 1966, where there were various...I believe it was called the "Iowa Summer Study," but the discussed the...a group of scientist got together and I think at that meeting they locked onto the three meter size for the primary. That is the way telescopes are generally specified, the size of the primary mirror. So we got into program development, we had an astronomy group established in my little Mission and Payload Planning Office. That group was headed by Jean Olivier. Jean began to...by the way, the fellow that I mentioned a moment ago, Max Nein [621] was also in that group working for Jean. But Jean really became involved in 1969 to start conceptualization of the large space telescopes. Just one of the concepts that we began to work on. I will just mention that we began to look at it initially through the Apollo Application kind of program, but really, to me the AAP was just to say, "hey we are using the technology that we have and we are going to do great and

new things." We started conceptualizing the Large Space Telescope when Jean Olivier group in 1969.

86. WARING: As part of AAP?

87. DOWNEY: Really as part of AAP, but really that was just kind of a buzz work almost.

88. WARING: Was that just a way to get your foot in the door?

89. DOWNEY: A way to think about...yes, we were doing a lot of things under the AAP banner. It wasn't totally a fictionalization. We were thinking about some of the structures and sizes of tubes and tanks and fixtures that we could be maybe used.

90. WARING: Were you thinking of launching it with a Saturn vehicle or just mainly using pieces of the hardware?

91. DOWNEY: No, we were thinking about at that time, launching either with a TITAN or with the Shuttle. I think the earliest workteam may have involved the TITAN system. I forget which TITAN vehicle was available at that time. But we were thinking of a vehicle in the thirty thousand pound class. It looks like, we again at Marshall tend to maybe in the payload world think big and large things. So we started to work on the Large Space Telescope in 1969 and that was one of the main endeavors of Gene's branch. As we went a little deeper in there we kind of dropped away from thoughts of the solar and any really active work on the solar system or solar telescope payload. It was not the solar system but the solar telescope, since HEAO was actively underway at that time, you know the gamma-ray and x-ray regime was being well taken care of. So we decided that we should really concentrate in this optical arena. The old OAO program, which I mentioned earlier was

much smaller system. It had brought along some marvelous technology, but it was just so much one could do with an OAO-scaled device. So Gene began to concentrate the attentions of his group on the optical payload, which we called Large Space Telescope.

By 1971, we had active propositions on the table to initiate a definition and development of a Large Space Telescope. We made a series of presentations to officials in NASA, particularly to Dr. John Naugle [664]. He was the Associate Administrator for Space Science at that time.

Within Program Development, not only was Gene Oliver group started to work, but then we reached into Program Development preliminary design group, Eric Gunter [671], now deceased, headed the Preliminary Design Group in Program Development. We had a matrix situation with Gene Oliver leading them, but we were drawing on a lot of people from Program Development in the preliminary design and concept development of the space telescope.

In 1971, as I recall, we formed a Task Team in Program Development. I headed that task team and we began to get the whole Center involved in developing the Large Space Telescope system. I should mention that in those early days we worked competing with Goddard...I better get this right. I ought to be able to peg this chronology! Do you happen to know when Petrone became Center Director?

92. WARING: I believe that was 1973 and he stayed through part of 1974.

93. DOWNEY: The Large Space Telescope Team was probably established in 1972 then, not 1971. Or maybe even early 1973, because I remember I had just been asked to manage that task team when Petrone arrived. I just didn't work in it as the task team manager. I had been involved with it since 1969. I had left the Mission and Payload Planning and had gone up to work on the ninth floor as kind of an assistant to the director of Program Development. Then I was asked to go back and work full-time as the program evolved

from the conceptual phase into a major definition activity.

94. WARING: When you were performing task team, that is when you began major efforts of drawing people from the labs to formalize the design?

95. DOWNEY: Yes, to formalize and complete the definition. So we had a major, major effort at the Center shortly after that task was created. The way that evolved was the work started in the old Mission and Payload Office under Gene Oliver's group. A little concept work was done there. Then the preliminary group within PD got involved and helped us flesh-out the engineering details of the concept and we got a little more mature in the definition. Then the task team was formed that brought in major support from various elements of the center as well as continuing support for program development.

Frankly, in recent years, the task team have not been staffed to the kind of level that they were staffed at Marshall in the early 1970's. As I recall, depending on when you check my office, we had some twenty-two to twenty-four people directly assigned to the task team project and being supported by ten times that many people throughout the Center. So what I am saying is that we had some kind of major project office organization in the task team, which was indeed a pre-project office to do the project work before the project was actually approved by the agency for development.

96. WARING: Can you talk about some differences and similarities in the way the Large Space Telescope developed in reference to the way HEAO developed?

97. DOWNEY: In the earlier work that we did on HEAO in the old EMR days was kind of "catch as catch can," and it didn't have any formal authorization or authority behind it. Although I should add, although it was done in SSL, Dale Ruth and myself went to other labs to get some help too. In that less restrained era we were able to get quite a bit of

enthusiasm and engineering support from other labs even in our early days of our work on HEAO.

Yes, there was a good bit of difference. When we got to the program development era in the late sixties, we...did I mention, I overlooked that we did compete rather vigorously with Goddard during the early days of the task team. Dr. Naugle and a group from headquarters went to Marshall and got briefings on our concept for a large space telescope. They went to Goddard and got briefings from the folks at Goddard.

98. WARING: So that was a difference...?

99. DOWNEY: It was a more, obviously, LST was more formalized. It was fitted into the mold of a project being defined by the Center. All the interactions were with Headquarters back and forth.

100. WARING: From the very beginning?

101. DOWNEY: From the very beginning. It was not a group of guys just freewheeling. So there was a big change.

102. WARING: Another difference was that Goddard wanted the project from the very beginning.

103. DOWNEY: Yes, they wanted the project from the beginning. Often, I think, projects...Centers can leap-frog over each other. I certainly wouldn't recommend that you record this, but my view, you look at HEAO, what happened? Marshall is so involved in HEAO I, II, III, that Goddard says, "Hey we need something that is a super HEAO." They called it the Gamma-ray Observatory. It will fly very shortly on the Shuttle. That was a

natural consequence of the evolution of the HEAO spacecraft, the GRO. By the way, Marshall has one-fourth of the science on GRO. A gamma-ray burst experiment being done by Jerry Fishman in Space Sciences Lab.

Back to my point, here Marshall was working so hard on HEAO. The project doesn't have the energy and opportunity to define the follow-on system. I think a lot happened in the same way to Goddard on the Space Telescope. They were so busy working OAO and other things, but so busy on OAO, that they didn't really get there act together to work the following system.

104. WARING: Right, once your plate if full, there is nothing more you can do, but science ideas need to keep going.

105. DOWNEY: I have never heard anyone else express that, but to me, that is the way that the Centers leap-frog projects. They say, "Gosh a lot of people will think that is just terrible, these Centers are competing with each other." But you made a good point. The science needs to go on. The ideas are there, they need to be given an opportunity to be realized and come to fruition. Not to lay in waiting for some Center to clear a project office plate and then think, "Well, what can we do next?"

106. WARING: Was that another difference between HEAO and the Large Space Telescope? Wasn't there a community of scientists already fairly well organized, wanting a three-meter telescope and putting pressure on NASA from the outside?

107. DOWNEY: Yes, I think that was a difference. The optical astronomy discipline was much more mature than the emerging x-ray and particularly gamma-ray disciplines in the early sixties. Think of that, x-rays had just recently...Riccardo Giacconi had just barely published a couple of his earliest papers and Herb Freeman as well. So that was just a

totally new discipline. The discipline of optical astronomy goes back centuries.

So you are quite right. Now, there was a great deal of debate in the optical astronomy community, I might add, of those that would say in the sixties that all the money should go to ground-based astronomy, this space business is just too expensive. So there was...I guess it was about the mid-seventies before I really felt that there was almost a total unanimonity in the optical astronomy community throughout the country that the space telescope was the thing to do.

Of course, the other side of the argument is that it doesn't work that way. If NASA didn't get the money appropriated from Congress to do the Large Space Telescope, you can't just say, automatically that money would have gone to the National Science Foundation or to somebody else to build telescopes.

So I think that the other side of the astronomers thought process saying, "Well, gee, we would like to have that money to do something on the ground." It doesn't work that way necessarily.

108. WARING: They were thinking that there was a fixed sum of money and you spend it on some guy, you are losing it.

109. DOWNEY: But I think that by the mid-seventies, honestly, there was...and Dr. C. R. Ardell [795], Bob O'Dell had a lot to do with helping that at my end of the line. But I think that was almost a unanimous agreement by all astronomers, all optical astronomers in the country. Certainly the ones that were more recognized and influential, that Large Space Telescope was a good thing to do. You are right, there was a very sophisticated and pure community out there, that was as early as the mid-sixties were thinking, "We need a big systems in space." A fair component of that community were already thinking in an organized way in the mid-sixties that we need a big system in space.

Of course, the difference in the x-ray guys, they can't see them from the ground, so

they have to go into space. The same for the gamma-rays, you can see gamma-ray showers and the results of them on the ground. But can't see the primary radiation on the ground. So there were a lot of differences

110. WARING: Were there similarities in the way the projects developed in the early stages, anyway?

111. DOWNEY: I think there were similarities too, in the way...of course I was involved in the early work at Marshall on HEAO or EMR as we called it and on the space telescope.

112. WARING: Was there a core group that worked the origins of both projects?

113. DOWNEY: Let me see who may have been in that core, besides myself. Mostly it was a little different set of people that worked the early work on the EMR and LST. See, we had gone through what had been a passage of what was about a half-decade. Reorganizations, people had moved around. Although a number of people at the Space Sciences Lab went with me to the new Program Development organization, there was a good bit of turnover. Hermann Gierow, by the way, was my Deputy down at the SSL division. Later he went with me as my Deputy to Program Development. So he shared that common ground. Hermann has a marvelous recall of the facts, too.

114. WARING: Yes we met him just to talk generally about Program Development, but not to talk about any projects in any detail.

115. DOWNEY: The guy that I mentioned, Max Nien, is still working for Hermann. But several of the people that came with Hermann from SSL had not worked on EMR. Really down there in the SSL during the EMR, we just had a handful of people driving it. Myself,

Dale Ruth, Hermann was involved.

116. WARING: That is another big difference then, the size of the early design team.

117. DOWNEY: Yes, we just had a lot more muscle when we went over to Program Development. A lot more people and muscle and so forth. But usually, one other thing I might add, usually ideas to me, don't come out of committees or big groups of people. Somebody usually meets down a spark someplace. But it sure helps to have a group of people that can pick up and drive that think forward. Pick out a good idea and drive it forward.

118. WARING: Could I ask you some more about the relationship between Marshall and Goddard. Why do you think that Marshall became the lead center on Hubble?

119. DOWNEY: I think that the point that I was expressing earlier and that I coin in my own mind, the leapfrogging of projects. Goddard had at that time again a very heavy plate of things to occupy themselves with. They had some very severe drains on their manpower. Marshall had put together an excellent team and had put great energies into the definition of the Large Space Telescope. I think that we probably impressed the people at Headquarters that we had the capabilities and experience to develop the Large Space Telescope. A principle in NASA headquarters at the time, besides John Naugle, was Jesse Mitchell, who worked under John as the head of the Astronomy programs for NASA. By the way, Jesse was not an astronomer himself, but he was an excellent manager and administrative. As I recall, he was an engineer that roots back here in Alabama. I am not certain of this at all, but I believe he might have been a graduate of Auburn. But, Jesse was the man up in Washington that we were interacting with. I think that we were able to just put more energy...I think what got the project for us is that we put a great deal of energy

and effort into the definition activity, I shouldn't say that...early conceptual definition activity. The interest of the Center Director in the area was shown to be considerable. Goddard, again, had their plate full as far as current assignments and work was concerned. So I think those were the two main factors. We put together a very strong proposal and Goddard had a large number of projects to do. Of course, I don't know how...I have not read this [Robert Smith's The Space Telescope] yet....

120. WARING: Yes, I have read that. It is an excellent book, I think.

121. DOWNEY: Bill Sneed just got me the copy. It deals a lot more with the later history and I was involved only in the early history. I stepped away from the project when it came through for development.

122. WARING: That was in 19...

123. DOWNEY: Nineteen seventy-six was when I left it. It was essentially approved them. I forgot how that exactly related to the final appropriations cycle that Congress went through. It went through a funny effort there and I assume that it is recorded in this book. Strange in the sense, we really got it approved through the Congress, but Congress said, "Well, we are going to wait a year to start." So the appropriations may not have come until the FY 1977. I remained with the Hubble only through the definition period.

124. WARING: Do you think in this case, in the case of the Large Space Telescope more so than HEAO, that NASA may have wanted to provide Marshall with a big science project that would keep its team together so that they wouldn't loose that expertise?

125. DOWNEY: Permit me to be a little cynical. I don't think so at all. I don't think that

the people in NASA headquarters, and please don't quote me directly on this, I am trying to...I don't think those people in Washington that were program managers and so forth, the people up there in Washington really think that much about the institutional affairs of the Center. That has always been one of my pet peeves throughout my NASA career. Because from my view, the Center has to go take care of itself and get its assignments.

126. WARING: I see, so the headquarter's people think just about whether the technical requirements of the system are...

127. DOWNEY: I mean, let's take the case of the Large Space Telescope. Jesse Mitchell, a brilliant guy and turned out to be a very effective manager, a good people person. He has a program he wants to fulfill. His name will go down in history as to how good was the NASA astronomy program in the sixties. That is what he is thinking about. He is not thinking about, "hey, those are good ol' boys down in Huntsville and we need to keep bringing them their plates!" He wants his program to look good. So he will go to the Center where he thinks he will get the best job done. I think that is what drives the Headquarter's line organization people. They have the line responsibilities for programs and projects. They want to get that work done in the most effective way they think that it can be done. They will go to the center where they think that it can be best done. In [some] circumstances, they might be say, "well, this center might really have a little bit better capabilities, but they are in no position to work this project now."

128. WARING: That is a good point.

129. DOWNEY: We could get into the engineering areas of point stabilization and control, for example. It is very common to most optical, space telescope systems, Marshall really got preeminent capability in that area, and has had for years. Of course that

capability grew up around our assignments and the assignments grew up around the capabilities. So there is a synergism there. So Marshall, I think, is well equipped to handle the scientific payload assignments that it has gotten. Another thing, unless there is just some major project on a national scale, like a lunar-Mars mission initiative or an Apollo Program, I have yet to see a project that was really created at NASA headquarters.

130. WARING: They come from the bottom up?

131. DOWNEY: They bubble up from the bottom, through the Centers. That is certainly my experience. Somebody could probably pick one where I am wrong, but I can't think of a one. Somebody might say that the idea of the Shuttle was George Mueller's idea, but I think Marshall was working on it for about ten years before I ever heard of George Mueller. I am sure that other people were thinking the same kind of things at other places.

So I believe that projects come from the minds of people and they typically flow upward. People in Washington, typically, are too busy day to day with the press of national [problems]...you know, the annual budget, the defensive this and that before the Congress, all the hectic pace things they do. Those people don't have time to think, only to react.

132. WARING: Right. I think that the history of NASA proves that NASA works better when the Centers are given flexibility.

133. DOWNEY: I think it does work better. I spent a little time, very little, spent a tour in headquarters. I went up there and pinch-hit worked for about a month and a half when the team manager was ill. Their life is hectic. They have to work very, very hard. They honor their own destiny. They yanked around all over the place. The OMB, the Congress and so forth. I was up there during a pretty active time in the budget cycle, I know that, but they don't have much time to really study and think and creative. Too frantic in the fire.

134. WARING: Can you think of, talking about the development of space sciences at Marshall in the late sixties and early seventies, can you think of things that we haven't covered that we should be thinking about as we write the next couple of chapters?

135. DOWNEY: Let me just think about the evolution of things in the Space Sciences for a minute. I don't have any real focus thoughts. The work in the early sixties the Research Projects Lab was very interested in microgravity area, as I mentioned. The effort shifted...by the way, microgravity area and radiation transport work associated with the use of rockets in the old Dr. Russell Shelton group that worked using the rocket shielding and other aspects of that program as well as some associated with science. Those interests, of course, have shifted. There has always been a pretty good group in the Space Sciences Lab that solar astronomy and they still work that area. They have done a lot of ground-base work there. But the emphasis in the current Space Sciences Lab is shifted into, certainly x-ray astronomy, gamma-ray astronomy, magnetospheres physics, particles and fields work. The microgravity program became prominent in the mid to late seventies.

136. WARING: By microgravity you mean material processing?

137. DOWNEY: Microgravity Materials processing. That work continues to be supported in the Space Sciences Lab. They have a division still dedicated to that. In fact, Bob Nauman, until he retired, headed that division.

There has been quite a transition in science interest in Space Science Lab. I don't believe...the lab is far more powerful today scientifically, through the many years they have evolved to a much more recognized and capability scientific organization. Not that we didn't have some excellent scientists in the early sixties. We just have many more of them today. It has just grown, as you would expect. I don't think Space Sciences Lab now, has

any group of people like my old group that was trying to conceive new scientific programs and projects, because as you are aware when Program Development was created in 1969, that provided that kind of forum to spearhead such projects. Of course, Program Development does work, from the days I was there until today, works with people in Space Sciences Lab.

The Earth Sciences are as gotten very important in recent years in the Space Sciences Lab. Earth Science and Applications Division. In the early sixties that group was mainly involved in tropospheric meteorology. Concerned with weather during the launch events and so forth. They still do that stuff but are looking at Earth Science in a much, much broader perspective; atmospheric physics, various aspects of sensors and...there is some remarkable technology that has emerged. I think they should have planned it earlier because there is a very appealing and worthwhile project that is being really spearheaded in NASA out of Goddard and through JPL. Marshall has a percentage of that effort as well.

138. WARING: What is this, Mission to Planet Earth? What is that; I haven't heard of that?

139. DOWNEY: Oh, you need to...it is the idea of making, preserving the earth's environment. Environmental observations from space to try and better understand the totally global system. Everything from better techniques for long-range weather forecasting to observing deforestation of tropical rain forests, erosion of costal areas and so forth. Marshall is particularly active in an area that involves the world's hydrological balance. You know, water...they aren't really talking about an inventory of water and oceans, but in the atmosphere. How is the water evolving...I can't articulate this stuff like a scientist...but what is happening in the tropical regions as the water is evaporated and the water re-circulates through the atmosphere. How the moisture and where it comes down, accounting for the rainfall world-wide.

140. WARING: When did this project begin? Is it a project?

141. DOWNEY: That was a project that has kind of evolved from many directions. It is a very popular project that been announced under the Reagan Administration.

142. WARING: Strangely enough!

143. DOWNEY: Strangely enough. But you talk about this stuff and it is almost like motherhood, and I am not saying this is not important to do, I think there has been a great...to me... and I don't follow such matters very well...but in the decades of the eighties, there began to be a national awareness more about the environment. Even the things about recycling and of course, the ozone depletion at the polar Antarctica ozone hole. Well, all these kinds of things that lead to a greatly awaken awareness on the part of almost the general public that, "hey, we have got to protect this environment." I don't that is any longer just a bunch of scientist that are running around espousing such things. I think the man on the street, the average intelligent layperson is getting concerned about our global environment. So I think that this Mission to Planet Earth, which is proposed as an international global kind of cooperative program, is something that is very hard to...you know Congress is supportive of it. They jump all over it.

144. WARING: Yes, that Earth Sciences work in NASA was very popular beginning with Skylab. Skylab had many earth science experiments.

145. DOWNEY: Right, but I think that in the Skylab era that was neat stuff, some scientist were very interested in it. But I don't think that there was a general....

[End of second side of first tape...271]

146. DOWNEY:...So I think that it came out under the Reagan Administration first, but I think, maybe, under the Bush Administration, they coined the title "Mission to Planet Earth." But shortly thereafter, Bush announced...this is what he called the Lunar/Mars Mission Initiative. The space and exploration initiative, kind of, for a while overshadowed the Mission to Planet Earth. But the Mission to Planet Earth Initiative is the one that will stay in power in my view. That is my speculation. That's going to be here. NASA is going to be spending a lot of project dollars in the Mission Planet Earth Initiative arena. Marshall has a fraction of that work.

147. WARING: Do you think that the Center is set up that it can expand its role in that program?

148. DOWNEY: I don't think that Program Development is working it nearly to the extent that it should. That is not a question of history to record in the history. That is just my belief. I believe that if I were in Program Development now and had the authority to expend some resources, I would be putting one heck of an effort into this Mission Planet Earth.

149. WARING: That is why I was surprised I had never heard of this. You hear about all other sorts of projects, but never this one.

150. DOWNEY: Yes. There is a group in Space Sciences Lab that has been rather quietly working in it. That emergence of that group came under Bill Vaughn, initially. He is now with the University of Alabama in Huntsville, also, Dr. William Vaughn. Then more recently after he retired, Dr. Gregory Wilson, a young atmospheric physicist, took over that

division in Space Sciences Lab. By the way, that division moved out of Space Sciences Lab and moved back in for some reasons that I won't attempt to try to explain. But for any rate they are back in Space Sciences Lab now. They have been very quietly and effectively moving right along. It is very interesting Earth Sciences work. Dr. Greg Wilson has now been moved up to NASA Headquarters to be Deputy of the...I think they call it...Greg Wilson is a real impressive young fellow, full of ideas and energy. I was trying to get that division title correct. They call it Earth Sciences and Applications Division of the Office of Space Science and Applications, NASA Headquarters.

No, the Mission to Planet Earth has been a quiet project at Marshall. But that group down at SSL has been working it. Herman Gierow's people have, Program Development haven't been totally without effort on that... I shouldn't imply that...Hermann Gierow has worked it a little bit with his people. But he has limited resources and hasn't really been turned on in a major way. So Marshall's role in that is going to be to get certain pieces of work that will fit in and dovetail into the larger effort being done at Goddard.

You may have heard the terminology, "Earth Observing System," EOS. The EOS is composed...that is part of this whole Mission to Planet Earth business...but in the EOS there were a number of platforms proposed...major platforms of polar orbit [045]. The Japanese have a platform proposed. The Europeans, as well as the U.S. It is a whole...it is really a major, major program now. The EOS will be a major new start for NASA in the very near future. It will be managed out of Goddard. But Marshall will also be involved in some of the science areas.

I got into that looking at some of the change in the SSL from the early days. Back in the sixties, I am not sure that I have really characterized that. One the one hand there was a lot of interest in the meteoroid environment, working electrical propulsion, ion propulsion, interest in the nuclear rocket program, radiation transport theory and applications. Now, as I mentioned, if you try to characterize today, the environment in SSL, resummarizing what I have said to try and collect my thoughts...we have the very

active programs in SSL with the groups working in x-ray astronomy, gamma-ray astronomy, cosmic-ray physics under Tom Parnell. We have the microgravity Division; the microgravity Materials Division. We have the Earth Science Division work that I have characterized mainly around the area of this climatology, Mission to Planet Earth kind of thing. Also now they have a very active program in the Space Sciences Lab in magnetospherics physics and upper atmospheric physics. I never know where upper atmospheric physics stops and meteorology begins.

So there has been a considerable transition in the areas of scientific emphasis, as you would expect.

151. WARING: Yes, there is a evolution from being an engineering/propulsion center to being an engineering/science center and the science is very diversified.

152. DOWNEY: Even in the earliest days of Engineering Propulsion Center, science was prevalent then early on in the Explorer I. Gerhardt Heller and is group in Space Sciences Lab, Gerhardt is deceased now. He was killed in an automobile accident a number of years ago. But, Gerhardt to me was...I have a lot of difficultly making the difference between some of the early thermal physics work that was done with radiation exchange...to draw a line of demarcation between thermal physics...developing some of the early theories of how objects will behave in orbit, where it is mainly radiating exchange. For example, in the cargo bay of the Shuttle it is radiating exchange, not convective thermal transfer. Gerhardt Heller and his associates did some of the early work on the Explorer I. I would say that they were thermal physicists, but they were doing engineering measurements as well. So that whole area has evolved down to where thermal engineering is done in the Labs and it is one of the major engineerings disciplines at the Center.

153. WARING: Can I ask you one big last question for us to end with? What difference

do you think it made in Marshall's Space Science activities that there were so many Germans involved in Space Sciences? Did it make any difference whatsoever, that so many of the scientists and engineers were Germans? Did they have a different outlook than American officials?

154. DOWNEY: I really don't feel...of course I worked so many years with Dr. Ernst Stuhlinger and closely associated with Gerhardt Heller. They were very...they were individuals just like anybody else. Dr. Ernst Stuhlinger was such a remarkable person and Gerhardt also. They...Ernst was so creative and tolerant of people to let them be...he had set up a very creative and an opportunity to let people be creative in his organization. But, hey, Ernst Stuhlinger was that kind of guy. He was born with certain amount of talents, energy and capabilities. If he had been born in Brooklyn, New York, or...I think he would still have been the same kind of guy. He was just a very extraordinary person. Of course, we inherited some of the best of the best when the Germans came to join us. Obviously Dr. von Braun was a unique person. So we owe a lot to the legacy and heritage of that group of talented people.

155. WARING: And it didn't have much to do with their nationality.

156. DOWNEY: I never thought much about that because I was so much thrust in the middle of it, I guess I would have to stop and reflect a second or two of who the Germans are and who they are not.

157. WARING: Do you think a lot of people felt that way?

158. DOWNEY: I don't know. You see I had some early-on...I was thrown...we had some U.S. scientists like Russ Shelton. Russ is still...I haven't seen him in years, but he is still

around. He was very talented and capable person. He epitomized a guy who wanted people to have a free environment in which to think and do things. So he was not a believer in a highly structured discipline organization. Again, it is just a little difference in the make-up of some of the managers. I don't know, if I could...I don't think that many of us out there in the middle of everything thought very much about whether these people were born in Germany or born in the U.S. Of course, their accents were never much like a native Alabamian, like myself, but I never every thought about it. Hell, it never made any difference. Really, I just never thought that much about it. Maybe the people that came with von Braun were picked and kind of formed through his own strength and personality and the kind of people that... maybe it was the fact of who was attracted to Wernher von Braun and to whom did he attract. Maybe that was the culture that came with those people, not just the fact of whether they were born in Germany or not.

159. WARING: They may not have been typical German scientists. They were von Braun scientists.

160. DOWNEY: Perhaps that is it. Because I noticed that often the people that gravitate around a leader, soon take on his personality if he is a strong leader...maybe pick up some of his...use him as a model, copy of some of the things that you do after that individual. In management of projects in the early days, I tried to take some of lessons from Jesse Mitchell at NASA Headquarters. I thought that he had some real good philosophical approaches to working with people and working projects. I said, "Hey that guy is pretty successful, so I think I will try to emulate some of this." But I never much thought about whether these guys were Germans or not.

161. WARING: Do you think many of them retired or were pressured to retire, depending on the way you look at it in the mid-seventies? Do you think that when they left the

organization in numbers, was their absence felt or was there so much restructuring going on at Marshall anyway, that it was hard to tell what was the consequence?

162. DOWNEY: Well I think that certainly they were missed. But I think that the younger guys had assimilated enough of that culture that you don't just turn off an organization's culture overnight. I think by that time it was deeply ingrained at Marshall. I can't say that they weren't missed. But I think that the culture that they brought with them remained at the Center and even remains today. So I do think that they were missed, but their leaving didn't destroy the culture, didn't destroy the organization.

The business of the scientist has nothing to do...I am somewhat distraught over the fact that NASA has apparently become a whipping boy in the eyes of the media. I don't know what one does about this. I think of the media as the antithesis of the kind of thing you do. They just kind of grasp this and print that. There is no research, no order and pattern of careful thought and trying to collect facts and be thoroughly about it. But this business about this ASTRO mission that has just been completed is really a [158]. That is a remarkably successful mission. I guess that ultimately it will go down in history, when all the science papers are published and see the returns from the science...boy that mission had a lot of complicated hardware and all of the experiments worked wonderfully well. Very sophisticated instruments developed at rather modest cost by some outstanding experimental groups from several universities. Sure, they are going to have...on the first few days of orbit they are going to have a little problem getting that telescope lined up and operating. It is a very, very sensitive and highly sophisticated system. But golly, they got some bad...you would have thought that mission was a failure if you read...particularly if you just read the headlines.

163. WARING: Why do you think that happened? Where did the press...at what point do you think the press loss affection and respect for NASA. Do you think with the *Challenger*?

164. DOWNEY: It has to be *Challenger*. I can draw a direct parallel with Spacelab 2 and ASTRO. Very similar missions. Space Lab 2 was a mission that used the same instrument pointer. Totally different science experiments. We had those troubles. When we first put Space Lab 2 on orbit, they flew in June of 1985 as I recall, pre-Challenger...we had some trouble. Of course, the press were much more interested, even then in the problems that we were having than in the successes, but about the third day, we finally got the thing running and it operating real well. The press got uninterested a bit when we solved all the problems. But then we finished that mission and we got just praise, it was a marvelous mission, the most exciting week in space ever! They just went on and on with the accolades. From what I understand, I wasn't out there in the Mission and Payload Operations Center, hell, the ASTRO mission was just about like Space Lab 2, that piece of Space Lab 2, a remarkably successful mission. The press...if you read the headlines, you would have thought the damn thing was a failure. I think that NASA has just assumed the role of a whipping boy in the eyes of the press. I don't know what one could do to break that situation.

Of course, I am greatly distressed and anguished over the problems that we encountered with the Hubble primary mirror. That should not have happened. That was a big and grievous mistake. But even that, the reports that came out said, well that nobody really knew that the Hubble test didn't really come out quite right because the information never got beyond the optical operations divisions of Perkin-Elmer in Danbury. They kind of contained their concerns and rationalized them away without notifying anybody. Then somebody concludes that is just symptomatic of the poor communications at NASA.

165. WARING: When it is being done in the private contractor.

166. DOWNEY: It was done by a private contractor. It said, well that is kind of the way

things were. It even went so far as to say at Marshall, they tend to suppress problems. They were told, well that is part of Marshall policy, you aren't suppose to move problems upward. That is absolute hogwash. If there was ever anything that Bill Lucas drilled into us, that was "if you have a problem, I don't want to be surprised please, please communicate it to me." Any manager that had any sense would be up there in a hurry anyhow. To take advantage of their experience. Of course they command some resources to help you solve that problem. That is absolutely asinine to think...you have seen that, it has been all over the newspapers, that some kind of culture at Marshall says that you shall not present..don't push problems up, we will solve them at the... Maybe somebody in the bowels of the Marshall organization may have had that perception at some point. But any responsible manager out there that has ever worked at that Center would say, "Man, one of my greatest fears is that I would have some kind of problem in a project I was running that I wouldn't properly perceive and Bill Lucas would perceive, (because he is damn smart)." My greatest fear was that he would find out a problem that I had that I didn't even know about. I was with him when I had a problem. Well, anyway, I am kind of off on a tirade about this media stuff.

167. WARING: Well, the press, their attention span is so short that if there is anything else that will attract their attention, they will forget about NASA, I think.

168. DOWNEY: Well, I really do think that NASA has assumed a whipping boy role and I guess you can see that happen throughout life. As early as grade school, playing around some kids, any walk of life, some people like to find some scapegoat that you can beat on and blame. You know that is a popular fun thing to do, lets whip on him. I am afraid NASA is playing this role too much.

Again, the terrible irony of the Hubble thing is that a tremendously complicated engineering associated with that overall observatory and the things that we felt so uncertain

about that worked out so well, so well, with all the new technology, the graphite epoxy structure and all that new stuff. The one thing that we thought the technology that it was, was the primary mirror. When I first heard that, I couldn't believe that was the problem. So that was just a grievous, human error that should have been caught. Can't deny that, but then to try to rationalize the error into some kind of communication thing like Challenger that happened at Marshall...I certainly have respect for people that do the kind of work that you do, that are trying to do a thoughtful research job on something, opposed to grabbing at anything and throwing it into print.

169. WARING: Well, this will make our book more difficult in the sense that there is an image out there about Marshall, an image that is often uninformed. We are trying to do a careful study. You have to confront those expectations that people have. But that is what makes writing history fun. Fun to correct myths and see things as they really are.

170. DOWNEY: [251] a terrible myth that these NASA centers can't work together. That is a terrible myth. Sure, if we are competing with Goddard for something like a space telescope, there is a strong feeling of competition, but once the competition is over, life goes on and you have to work together.

171. WARING: The competition is mainly in the A and B Phase, right?

172. DOWNEY: The define for a project assignment. Like in Spacelab missions, like in ASTRO, the engineering is done at Marshall. The payload is put together physically integrated at the Cape. The flight, until this ASTRO mission, the flight...of course the overall flight control mission operations is out at JSC. Even until this last mission, the payload operations were conducted out of JSC, payload guys went to JSC to do it. This most recent mission was done here. Of course the flight crew comes from JSC, obviously

the astronauts and most of the training is done at JSC for the flight crew, including the payload specialists. So all these centers are working together all the time. On the Spacelab, I am just using that as an example, it is just a way of life, the reason we shared it was some politics that they didn't want to put everything in one place, just kind of spread the action around. One could argue that in some cases that is not the most effective thing to do. But, I think that the centers have proved that they can work together very, very effectively. It is a pretty complicated program. I think that ought to be, to me that is one of NASA's great strengths, not one of its weaknesses.

173. WARING: That is something I will try to bring out when I talk about the Skylab. Initially there was a lot of competition, but that was resolved. The way the centers worked together to rescue the Skylab when there was damage.

174. DOWNEY: Marvelous job. That is right. NASA is very capable of pulling together and working the problem.